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IN THE UNITED STATES DISTRICT COURT
FOR THE SOUTHERN DISTRICT OF OHIO
EASTERN DIVISION AT COLUMBUS

CITIZENS AGAINST POLLUTION,
Plaintiff,
vs. Civil No.: 2:04-CV-00371
Judge Gregory Frost
Mag. Judge Mark Abel
OHIO POWER COMPANY,
Defendant.

D E P O S I T I O N O F

WITNESS: STUART BATTERMAN, Ph.D.

LOCATION: Offices of Patricia Murray & Assoc.
122 South Main Street
Ann Arbor, Michigan

DATE: January 5, 2006

APPEARANCES:
FOR PLAINTIFF: BAILEY & GLASSER
227 Capitol Street
Charleston, West Virginia 25301
BY: BENJAMIN L. BAILEY

FOR DEFENDANT: PORTER, WRIGHT, MORRIS & ARTHUR
41 South High Street
Columbus, Ohio 43125
BY: MOLLY S. CRABTREE

Reporter: Karen Klerekoper, CSR-4250, RPR

I N D E X

WITNESS: STUART BATTERMAN, PH.D. PAGE NO.

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Ann Arbor, Michigan
January 5, 2006
At 2:10 p.m.

STUART BATTERMAN

HAVING BEEN CALLED BY THE DEFENDANT AND SWORN:

MARKED BY THE REPORTER:

DEPOSITION EXHIBIT NUMBER 1001

EXAMINATION

BY MS. CRABTREE:

Q. Good afternoon, Mr. Batterman. Could you please state your name for the record.

A. Stuart Batterman.

Q. Mr. Batterman, Dr. Batterman, where are you employed?

A. I'm employed at the University of Michigan.

Q. How long have you been there?

A. Since 1989.

Q. I have marked, just for the record, the protective order in this case, which was previously marked as 1001. This is not something that you would have necessarily seen before, but we mark it just in case, in all of the depositions, in case there is any sensitive business information, either for you or the defendant in this case, that we have an opportunity to mark that later as confidential.

Have you ever been deposed before?

A. Yes.

Q. How many times?

A. At least three, I think, maybe four.

Q. What were the circumstances of those depositions?

A. Those are different cases dealing with environmental matters.

Q. What type of environmental matters?

A. Pollution cases, for the most part.

Q. Anything dealing with sulfuric acid?

A. Sulfuric acid was involved slightly in one of the cases. It was mostly S02.

Q. Can you describe the circumstances?

A. Sure. In the December period, I think of '95, a large pile of elemental sulfur caught on fire, and the combustion products included sulfur trioxide and some sulfuric acid as well.

Q. Where was this?

A. This was in Macassar Township, in South Africa.

Q. What was your role in that case?

A. I was an expert testifying before a presidential commission of inquiry with respect to the establishment of negligence in that case.

Q. What were your other depositions about?

A. I have had depositions dealing with a wood waste

energy facility located in Flint. I have had depositions dealing with a cement facility burning hazardous waste in Texas, and I have had depositions dealing with a cement facility in Camden, New Jersey.

Q. Suffice to say, you have pretty good experience with depositions. I just want to go over a couple of rules to make sure we are on the same page.

One, as you know, all of your answers have to be oral. A shake of head isn't caught by the court reporter, since she is taking everything down.

Two, if you need a break at any time, just let me know. We are not here for an endurance contest or to make you uncomfortable. I just ask if there is a question pending, you finish answering the question before we break.

Third, if I ask anything that doesn't seem clear to you or you don't understand, just ask me for clarification now so we make sure we are both talking about the same thing now as opposed to misinterpreting it down the road.

Next exhibit I will mark as 1075, I believe it's a copy of your resume.

MARKED BY THE REPORTER:

DEPOSITION EXHIBIT NUMBER 1075

BY MS. CRABTREE:

Q. If you could take a look at that. I gave you the wrong one.

MR. BAILEY: The one with your highlights?

MS. CRABTREE: Yep.

MR. BAILEY: I would be glad to take that.

MS. CRABTREE: I don't think there is too much interesting on this one.

BY MS. CRABTREE:

Q. Could you just take a quick look at this and let me know?

A. It looks like a copy as of October '05.

Q. Has anything changed since October of '05?

A. Yes.

Q. What has?

A. There is some new publications and some committee assignments.

Q. What are those publications about?

A. Those publications are dealing, I believe, with volatile organic compounds in the environment. I believe one of them is an epidemiological paper. And I think the others are essentially changes from submitted to being accepted, minor stuff like that.

Q. Anything relating to sulfur trioxide or sulfuric acid?

A. No.

Q. Can you briefly -- this is obviously quite the

extensive resume. Can you briefly go over your educational history for me on the record?

A. Sure. I have a BS, bachelor of science, from Rutgers University in '79 in the environmental science department. MS and Ph.D. degrees from MIT, Massachusetts Institute of Technology, in '81 and '86 respectively. That's the formal education training.

Q. This appears to be a complete copy of that. I think we can probably move on from here. I'm not going to drag you through all of it.

Tell me how you came to be retained --

A. One other --

Q. Oh --

A. -- change on the resume I added here. Currently I'm also a professor of civil and environmental engineering at the University of Michigan, as well as being professor of the environmental sciences.

Q. Has that been a recent appointment?

A. That appointment initiated January 1 --

Q. Congratulations.

A. -- 2006.

MR. BAILEY: Or condolences. We aren't sure.

MS. CRABTREE: It's perspective, I guess.

BY MS. CRABTREE:

Q. Can you tell me when you were retained by the Citizens Against Pollution?

A. I think we had some discussions in the early part of 2005. I don't have a precise date available, to my recollection.

Q. Who contacted you?

A. I believe it was Jim Hecker.

Q. Who all have you had contact with since 2005 in relation to this case?

A. From the legal staff?

Q. Sure.

A. Just Jim Hecker and Mr. Bailey here.

Q. What about nonlegal staff?

A. During a plant visit, I met some of the folks at the Gavin facility and also a few other attorneys working with Jim Hecker and Mr. Bailey here, and I met Phyllis Fox at one of those meetings, and that's all, to my recollection right now.

Q. Was that the only time you spoke with Dr. Fox?

A. No, there was some telephone calls I think that were conference calls where she was a participant.

Q. Was that during the preparation of your report?

A. It was in that time period, yes.

Q. Did you review one another's draft expert reports?

A. No.

Q. What is your understanding of your role in this litigation? By this litigation, I mean the Citizens Against Pollution versus Ohio Power Company.
A. My understanding is that I was to provide an opinion regarding the exposure and health effects of the sulfuric acid emissions at this facility; that I was to examine the likelihood that the Gavin facility was the responsible agent or party. Those are the primary objectives.
Q. When you said the likelihood that the Gavin plant was the responsible agent or party, responsible for what?
A. Responsible for the concentrations of sulfuric acid and the health effects of the local community members.
Q. Next I'm going mark 1076. This is the Notice to take your deposition today and tomorrow.
MARKED BY THE REPORTER:
DEPOSITION EXHIBIT NUMBER 1076
BY MS. CRABTREE:
Q. Have you seen that before?
A. Yes.
Q. What did you do to prepare for your deposition today?
A. What did I do to prepare for it today?
Q. To prepare for today's deposition.
A. Okay.
Q. We are going to give you a chair on the English

department next.
MR. BAILEY: Law school.
A. I have done several things. I reviewed expert reports that were available to me. "Review" means a fairly quick review of them. I reviewed my report and I had a telephone discussion with the attorneys with Jim Hecker and Mr. Bailey here.
BY MS. CRABTREE:
Q. About how long did you spend preparing for your deposition?
A. I haven't calculated the hours, but I have had many other things to do this week. So, in total, it's probably -- I haven't actually summarized the hours, but I will, because you will see that, but approximately a day.
Q. Okay. Now, how long did it take you to prepare your report, your expert report in this case?
A. The record will show that I billed for approximately 124 hours, I believe, on the order of three-some weeks.
MS. CRABTREE: Next housekeeping business, 1077.
MARKED BY THE REPORTER:
DEPOSITION EXHIBIT NUMBER 1077
BY MS. CRABTREE:

Q. This is the subpoena. The next document I will show you is the subpoena for documents that was served on you in December. Actually, it was sent to Mr. Hecker on your behalf. Can you take a look at that and tell me if you have seen it before?
A. Which document do you want me to look at?
Q. The subpoena. The other one is the protective order that we marked, that had been previously marked.
A. Okay. I haven't seen the protective order.
Q. I don't think you need to go through it. If you want to, you're fine.
A. I have no idea what it's about.
Q. Okay. I'm not going to ask you any questions about it.
A. All right. Yes, I have seen this document before.
Q. Did you prepare a response to this subpoena?
A. Yes.
Q. What did you do to prepare that response?
A. I made a computer copy of my directory with all of the files related to the work product, my expert report. I culled my e-mails to find all of those that were related to this report, made a copy of that. And for materials which were not available electronically, I produced a copy, two copies, and I sent them to Mr. Hecker.

Q. You said you had one file on your computer where you kept all of this. Is that typical for you, with a project, to keep all of electronic documents in one place?
A. Yes. Well, no. Actually, I keep them in several computers, but they are all maintained, and several backups sometimes, but they are all in one directory.
Q. I notice --
A. At least that's what I try to do.
Q. Me too.
I notice in your report you mentioned that you provided your services at a reduced rate. Why is that?
A. For cases that I believe are social-justice related types of issues for nonprofit organizations, I do want, and I feel it's part of my responsibility as an academician to provide, to be able to provide services to those organizations.
Q. How often do you do that?
A. At a reduced rate?
Q. Yes.
A. Routinely for these types of cases.
Q. Would you say that you take on more than one project a year?
A. No, less than one project a year.

Q. A project every two or three years?
A. Probably average something like that.
Q. Okay. What is your normal rate?
A. The normal rate is approximately $300 an hour.
Q. Did anyone assist you in preparing your report?
A. I may have had some minor assistance in the form of obtaining a article, or something like that, from one of my staff, but no one did anything beyond the level of clerical assistance or library assistance.
Q. So nobody provided substantive feedback for you on any of your drafts?
A. No.
MARKED BY THE REPORTER:
DEPOSITION EXHIBIT NUMBER 1078
BY MS. CRABTREE:
Q. Exhibit Number 1078 will be a copy of your report, a color copy of that. If you can take a look at that and tell me if it looks like an accurate copy and complete copy.
A. It does.
Q. Can you turn -- I would like to turn first to section 2.2.
2.1 is a summary of your opinions, and lot of those reappear in the substantive sections; is that accurate?

A. Yeah. The summary section should be a verbatim copy, I think, material that is italicized in other sections.
Q. That's what I thought. If we can go ahead and turn to 2.2, then. We will start there. This is titled Ambient Concentrations of H2S04 in the Community. The first nonitalicized paragraph in this section, you reference Table 1 and say that it lists ambient concentrations of H2S04 detected in the vicinity of the facility in 2001, 2002.
Could you flip to Table 1 with me? I have a couple of questions on that. Are you there?
A. Yes.
Q. Okay. Now, these are a listing of all the detectable concentrations of H2S04 from the ambient monitoring; is that correct?
A. That was the intention of assembling this table, yes.
Q. Why did you not include the nondetects?
A. I didn't include the nondetects for a couple of reasons. I was interested in understanding where plume touchdowns were detected, not necessarily where they were not detected.
Second, the nondetects, as well as the detects, are listed in other places and are rather voluminous.

And, third, the ability to draw some inferences from the monitored concentrations is really only a function of where they are detected. In other words, I can stick a monitor in lots of places and I know I won't find a plume touchdown.
Q. Did you consider the nondetects informative to your opinion about exposure in the community?
A. They are informative only in regard that it provides some information with respect to the intensity of the monitoring effort.
Q. What do you mean by the intensity of the monitoring effort?
A. The strategy and the success of the strategy that is being used. It's useful to know where and how often a monitoring was attempted.
Q. Did you ever view any protocols for the ambient monitoring?
A. Yes, I have.
Q. A list of sites where the ambient monitoring was done?
A. Yes, I have.
Q. Do you have any opinions about how that ambient monitoring was done?
A. Opinions regarding what?
Q. Whether it was sufficient, whether it was correctly done?

A. What do you mean by correct?
Q. Well, you said you considered the nondetects informative as to how the monitoring was done. Do you believe that the monitoring was done effectively?
A. I don't know what you mean, "effectively." The monitoring appears to have been done from the -- I'm going to separate the monitoring in a couple of components.
Q. Okay.
A. The monitoring done by the consultants Lawhon, or Lawhon -- I'm not quite sure how to say the name.
MR. BAILEY: No R, Lawhon.
A. -- appeared to follow a standard protocol that was adequately performed in terms of collecting a one-hour, six- or eight-hour average.
BY MS. CRABTREE:
Q. What about the 2002 monitoring?
A. Again, the consultant performing the data sampling appeared to collect samples, to the extent of the knowledge that I can see, using, again, standardized procedure, very simple procedure.
Q. Would you have done it differently?
A. Most likely.
Q. How would you have done it differently?
A. As I stated in this report, I have not designed a

sampling strategy for this case. I think it would be done quite differently. The key idea is to recognize that plume touchdowns are intermittent and can occur over a large spacial area. And it's very difficult to capture them with a short grab sample, or a short time sample, and thinking about the strategy which would provide effective determination of concentrations and exposures would probably lead to a design which would be a bit different, yes. And I haven't -- as I stated in the report, I haven't designed such a strategy for this facility.

Q. So you don't necessarily have an opinion about the better protocol that would be followed; you just think you might do it differently?

A. I have a number of opinions about better protocols.

Q. What are those?

A. Well, there are a number of deficiencies in the current protocols. And they include inadequate temporal and spacial coverage, the difficulties with respect to averaging times, detection limit issues. And then from the standpoint of understanding sources, the simultaneous collection of additional information that would help determine both the origin of the materials monitored as well as the prevailing meteorological, and other conditions that might affect

concentrations that were monitored.

Q. What additional information do you think should be included?

A. A videoed record is useful, local meteorological measurements are useful. Other pollutants can be very diagnostic as well.

Q. By other pollutants, you mean things like SO2 and NOx?

A. SO2 and NOx would be useful.

Q. You mentioned difficulties with the averaging times, what were those difficulties?

A. The protocols, and one thing I neglected to mention in terms of deficiencies were the paper record with respect to the samples and their analysis, but a tighter trail of data completeness and characterization would be beneficial and is fairly normal in these kinds of incidents.

Your question was what?

Q. What was the difficulty in averaging time?

A. The averaging time, as you are aware, were three to eight hours in the 2001 time frame, most being at the longer end, roughly six hours. However, the data record doesn't explicitly state in all cases what the averaging time was, and that's one of the difficulties.

In 2002 the consultant shifted to one-hour

samples, which involve a couple of issues, as I discuss in the report. One is the ability to compare them to the 2001 data using a longer averaging time.

The second has to do with detection limits. A third has to do with the likelihood of actually capturing a plum touchdown with a short-term sample.

Q. Is that difficulty with the averaging time why a lot of these values in this Table 1 under duration for the 2001 area -- or for the 2001 time period, don't have anything listed for duration?

A. I could not locate duration in the available record to me.

Q. Now, I notice 10 of the 14 measurements -- I think it's 14 measurements, from 2002 were from June of 2002. Did you find that to be an anomaly from the previous year?

A. No, there were measurements in 2001 that were detected during the June period as well.

Q. But the fact that the majority of the 2002 data was in the June period didn't strike you as odd?

A. Well, I'm aware that there are issues in the 2002 data that deal with some background contamination.

Q. The two September and one October data points of 2002 in this chart, we were not able to locate any of that data in the sources that you cited for the table. Do

you remember where that information came from?

A. I'm sorry, which dates precisely?

Q. In 2002, September 11, September 16th, and October 25th.

A. I tried my best to provide the -- what are they called, Bates numbers?

Q. Uh-huh.

A. That's where I believe they came from, and some of these spreadsheets are quite large and have multiple pages and aren't very well organized or documented, and they may have been in there.

Q. Okay. It might just be an artifact of us not having pinpointed it yet.

I'm going to show you a couple of examples of the Bates-numbered pages that you have in here to make sure I'm clear on what you were looking at. Can you grab me AEPGVA 256 to 258. This is just a portion of one of the documents that you cited.

Thank you.

MARKED BY THE REPORTER:

DEPOSITION EXHIBIT NUMBER 1079

BY MS. CRABTREE:

Q. If you can take a look at that. Now, this is a portion of one of the documents cited in your notes to Table 1. This is the type of data that you relied on

in putting together your Table 1?
A. This is a table that is not a type of data.
Q. Was this type of information that you relied on in putting together your Table 1?
A. This is some of the data that was available, and whether this particular page or not was used, I don't know offhand.
Q. That's fair. There is a lot of pages I'm sure you went through.
A. This is a summary in addition -- the laboratory performing the analysis had more details in terms of the date and timing analysis that was performed; in some cases, the technician performing the analysis, and ancillary information like that. I think I might have an example of the laboratory sheets.

MARKED BY THE REPORTER:
DEPOSITION EXHIBIT NUMBER 1080

BY MS. CRABTREE:
Q. This is AEPGAV 36 through 39. Is this the laboratory information you were referring to?
A. Yes, and there is also -- this is still a summary of the information, and there are handwritten entries both from the field, I think, as well as from the laboratory, I think. I may have attempted to try to consolidate all of that information.

Q. Those are the handwritten data sheets?
A. Portions were handwritten in fields.
Q. You said you were aware of some contamination issues with some of the 2002 data. What is your familiarity with that?
A. As can be seen on some of the materials that you have handed out, they indicate some of the blanks were non -- nonzero; in other words, they had positive artifacts on the concentrations of sulfuric acid that were apparent on what are either laboratory or field blank. It's unclear from the available record.

There is also a string of e-mails and short reports from -- what's her name -- Bechtle, and an analysis, I think performed by her and reviewed by Lytle, I think discussing the blank contamination issue and the actions to be taken.

Q. Did that change your opinion in any way?
A. My opinion of what?
Q. Of the concentration levels in 2002.
A. Well, it makes them more difficult to interpret. You have the possibility that the levels here are not blank corrected or blank corrected. It was never clear to me.

The procedure that the laboratory is using, I believe one of the documents indicates that they do

use a blank correction. And in which case, essentially, the blank is subtracted from the measurement prior to calculating a concentration. This was never clear in terms of the procedure that they used or in the data.

It's also unfortunate and an inappropriate presentation of the data not to flag values that are potentially problematic. Typically most laboratories will have fields where they indicate a quality measure and flag whether or not there is a problem with that. Almost ever accredited lab will do something like that. My lab routinely does that.

The periodic reviews of the data and the analysis as they come in performed in this case, like in most labs on a weekly basis, also didn't generate any corrections, flagging -- or flagging of the data on a routine basis. So it's difficult to interpret how they felt about the analysis.

The analysis that was performed, I think, characterized the contamination issues as sporadic and, thus, applying some likelihood or some probabilities the -- most of the samples would be unaffected, but that's unclear to me as well.

So the analysis of the 2002 data for a portion of the time period could have a sample which

represents a true value, a true value plus a blank contamination issue, a true value with a blank correction subtracted, if the laboratory did that, or just a blank contamination issue. And I'm in little position to judge, as I think we simply don't know.
Q. When did you become aware of the problems with the -- the potential problems with the 2002 data?
A. I had noticed a blank contamination issue when I went through the data in the preparation of my report. However, at that time I did not provide a systematic analysis of blanks, because as I stated, in most cases, invalid data would be either flagged or otherwise noted.

The fuller awareness of the blank contamination issue was in discussion with Jim Hecker and subsequent review of, I think her name is, Deborah Gray's report, expert report. And that was in -- in just recent days.
Q. Recent days, meaning the past couple of weeks?
A. Yeah. It was subsequent to my response to your document request.
Q. When you noted some of the problems when you were going through the data in preparing your expert report, why didn't you note in your report that there were some potential problems there?

A. I didn't provide, as I just stated, a characterization of the blanks, and didn't feel at the time, and I had seen no other indications at the time, that this was a widespread problem, so --
Q. Did you --
A. Excuse me. I also did not go through the 2001 data with the same level. I mean, generally when data are provided, you accept the data for what they are, and 2001 data had no other indication either that there was a data quality issue, either good or bad, with that data.
Q. So you treated --
A. Finally, I believe this is an accredited laboratory and I do expect that they have a management system in place that both corrects problems rapidly as well as provides appropriate documentational problems.
Q. Anything else?
A. That's all I can think of.
Q. I don't want to cut you off.
So when you were putting together this table, you treated the nondetects similarly to the blanks and you didn't include them in the table? Excuse me, the blanks similarly to the nondetects, and you didn't include them in the table?
A. I didn't provide any analysis of blanks in this table.

As I indicated to you, it's unclear whether we have a laboratory blank field blank, and so forth, so I guess my fourth reason is that the lack of documentation available to me would make an analysis rather difficult.
Q. Well, let's go head and turn to tables 2 and 3, which is the next page in your report. If you can take a look at these for me.
Table 2, I think this is just a different way of asking what I asked you before. At the bottom there, you have the number of observations. By that, do you mean observations of a detectable level?
A. Yes.
Q. Okay. That wouldn't include any of the nondetects, correct?
A. No.
Q. Also, if you could flip back to Table 1, many of these sampling sites -- I'm looking particularly at the 2001 data because there seems to be more of it -- a lot of these are on plant property, correct?
A. Some of them certainly are, yes.
Q. Can you tell that for some of them by looking at the description?
A. Sure.
Q. Those are included, though, in your Table 1 as far as

number of observations.
Did the fact that some of these detectable levels occurred on plant property affect your opinion about community exposure?
A. No, not really.
Q. Why?
A. First of all, the detections that are available represent the situation where the monitoring team is able to get to a site in time when the plume may be present and, thus, recorded a detectable level. This is most likely to be the case where they have access to the site, and so -- that, or close to the facility. So it's just logistically much easier for the team to get out to a site on plant property.
Second, a wind shift of a few degrees, a gust here or there, may shift the plume to be off-site property but, in fact, not have a significantly different concentration. And I think that is reflected in the totality of the evidence that is here, as both with respect to the monitoring as well as with respect to the modeling that is performed.
Q. Do you know the percentage of 2001 or 2002 samples that actually showed detectable levels?
A. I did at one point. I'm sure you have it.
Q. I don't, actually.

A. It's I believe -- it's a percentage in the single digits, but I don't recall the exact number.
Q. Did that affect your opinions about exposure in the community at all?
A. Not really.
Q. Why?
A. As I indicated, the ability to place a monitoring team at the appropriate space -- at the appropriate place, rather, at the appropriate time over a large region is quite poor. And most attempts to try to get at plume detections for such episodic events are failures. This is nothing secret, nothing unusual. It's been shown in additional studies that the ability to do this is quite poor.
This includes some quite drastic cases where, in fact, a team believes they are at the correct spot and, in fact, has adverse health impacts even at the time. Yet the monitor shows nothing for various reasons.
So it doesn't surprise me that sort of a monitoring strategy using a mobile team, predesignated locations, for monitoring with an episodic type of event shows, in fact, that there is a very small percentage of observations which are what I would characterize as hits.

Q. You mentioned predetermined locations, did you look at how those locations were chosen?
A. I think I reviewed that. I think I have seen that for the 2004 data -- 2002 data. For the 2001, I don't recall offhand.
Q. In your table 3 here, on the left-hand side, you have touchdown duration. Where did these come from?
A. This is simply a sequence of durations from a quarter hour to two hours to demonstrate the point of the table. So these aren't coming from observations, per se, from objective observations.
Q. Are these what you consider reasonable durations for plume touchdowns?
A. I think this is a reasonable range. Whether or not a duration is two hours, or whether there is one that is a quarter hour, I don't know. What I'm trying to provide here is a reasonable range of durations, something that we call a sensitivity analysis.
Q. What did you base that reasonable range on?
A. In part, the frequencies of plume events that I have seen in other circumstances, frequencies or durations reported by individuals, video documentation. I tried to cover a large range here.
Q. You mentioned other plume events that you have seen. What other plume events have you seen?

A. From other facilities not in the Ohio/West Virginia area, but other emission facilities with tall stacks emitting sulfur dioxide or other gases that are easy to measure.
Q. What about sulfuric acid?
A. No, these didn't include sulfuric acid.
Q. Have you ever seen what has been referred to in a lot of these documents as a blue plume?
A. Yes.
Q. What were the circumstances of that?
A. I made a site visit, as I mentioned, and there was a tinge of blue, perhaps, in the plume. I have also seen videographic evidence that I have mentioned.
Q. But other than that site visit, you have never seen a blue plume from either the Gavin or another facility?
A. I don't recall seeing or noting other -- I look more carefully these days.
Q. Also, in Table 3 where it says the 2000 sample period in hours, you have the lowest average and highest. If you were missing the sampling duration for all of these samples on the previous page, where did you get the statistics about the sampling periods?
A. Okay. First, I believe you are referring to the 2001 sample period, not the 2000 that you stated.
Q. Yes, I'm sorry, 2001.

A. I have to think for a second where I got that from.
I believe what I tried to do was look at recorded durations. Then, since they were incomplete, I think what I did was to assume certain sampling rate of -- I forget exactly. It's in the 50 mills per minute, or something like that, and then derived it from the sample of volume which, in fact, was recorded for the majority of the samples.
Q. So you inferred the sampling period then?
A. I think that's what I did. I think I also discussed exactly what I did in the text, and if you will let me refer back to that?
Q. Sure.
A. Page 7, the bottom paragraph, on the fifth line, by examining the monitoring logs sample durations of 11 of the samples were identified as shown in Table 1. These average 5.7 with the short outlier omitted. The other sampling durations were -- I'm paraphrasing -- were unavailable. Typically sampling volume over this period was 96 liters based on the average of 38 samples.
And given this average sampling volume, it would be between three and eight hours based on the minimum/maximum flows allowable for the NIOSH method. That's where it was derived from.

Q. Okay. That's helping me. I think I know the answer to this, but on Table 3 in these averaging times, you did not include the durations for the nondetect samples; is that true?
A. For the averaging times for what, I'm sorry?
Q. The samples where there was no concentration detectable.
A. For which year?
Q. 2001.
A. 2001? That's correct. This is based on the detected data; however, I think I did show, in fact, that the nondetects at various similar sample volumes, so I don't expect there to be much of a change.
Q. Okay. That was going to be my next question.
We can go back to page 6 of your report. I think I'm done with the charts. The last paragraph on that page, you reference a document with a Bates number AEPGAV-E 19844 as saying that both Gavin units 1 and 2 with stated SO3 stack concentrations, 35 to 40 ppm.
I'm going to have you take a look at that page, what is part of, what looks like a PowerPoint presentation.
MS. CRABTREE: Can you mark that, please.
MARKED BY THE REPORTER:

DEPOSITION EXHIBIT NUMBER 1081

BY MS. CRABTREE:

Q. I include the whole presentation for context, October 2002. It is the last page of this, 19844. It's not terribly easy to read.

Is this where you got this 35 to 40 ppm per stack of SO3 concentration?

A. On the last page, it has the number 35 to 40. It's cited in the report. So I assume that's where I got it from.

Q. How did you know this was per stack and not the combined stacks?

A. I don't know that this provides that information. I don't recall right now whether I had looked at some additional information to make that determination or not.

Q. Fair enough. On footnote 2 on page 6, again, this time you used a stack concentration of 37 1/2 parts per million at both units in 2001. Do you know where that assumption came from?

A. I think what I did here was assume a range of 35 to 40 and just take the average of that range, so I used the midpoint, and I believe that's where I got 37.5 from for 2001.

Q. Then you go on to say: 14 parts per million at Gavin

2 in 2002 and 6 parts per million at unit 1 in 2002.

Do you know where those numbers came from?

A. Off the top of my head, I don't; however, I had seen description of emissions at unit 2 and at unit 1 for 2002 as well, so that's where it was taken from. I did not cite the source, which I should have --

Q. Okay.

A. -- to refresh my memory for that purpose.

Q. Would it have been -- would the information you base that on be in the documents that you provided in response to the subpoena?

A. It should be, yes.

MS. CRABTREE: Let's take a short break.

(A recess was taken)

MS. CRABTREE: Back on the record.

BY MS. CRABTREE:

Q. In section 2.2.1, which starts at the top of page 7, you say that several of the site locations for 2001 are unavailable. This is in the second line of the first nonitalicized paragraph. Again, how does not knowing the site information for the plume touchdown factor in your opinions about community exposure?

A. Well, I would like to know as much information as possible about the plume touchdowns, and the site where the touchdown occurred is, of course, a key

piece of information.

If I don't have that information, then I'm -- I simply don't have that spacial coordinate, and that is unfortunate, and I was unable to plot those locations on the maps that I provide as figure 8 -- I think it's actually earlier than that, as figures 1A and B -- so I was not able to locate a couple of the detected sulfuric acid concentrations, so they simply didn't show up on the map.

Q. But that doesn't necessarily change your opinions about community exposure to the plume?

A. No. It would be nice to have more information, of course, but the omission of an observation, or two or three, doesn't substantially change the picture.

Q. Also, in figure 2 -- and you reference this in the second nonitalicized paragraph on page 7, figure 2 shows the wind direction, and you use meteorological data from Parkersburg, West Virginia. Did you consider using any other source than Parkersburg?

A. I think that's a compound question.

Q. Did you consider any other meteorological data other than Parkersburg?

A. Yes.

Q. What other sites did you look at?

A. I don't recall all of the sites that I looked at. I

looked at the available data from the on-site meteorological station at the Gavin facility, as I stated in the report, and I believe I also looked at several other sites where data were available that I could get at on the Ohio EPA website.

Q. Did you look at Huntington meteorological data?

A. I think Huntington was one that I looked at.

Q. What about Pittsburgh?

A. I don't believe I spent much time with the Pittsburgh data.

Q. Now, looking at your figure 2, does this tell us the predominant direction that the wind is blowing?

A. This is a wind rose that shows us the joint probability of wind speed and wind direction over, in this case, an 80 -- I think it's an '88 to '92 time period. I can't quite read -- in any event, it's a multi-year time period which has at least five years in it, I think. I don't know if you can read that.

Q. I think it is '88 to '92.

A. '88 to '92, essentially '88, '89, '90, '91, '92. It's typical to assemble these things for five-year time periods.

This is showing only the winds during the April-to-October season, and only from 7:00 a.m. to 6:00 p.m. season. And this was -- this analysis was

provided, as stated there, from an EPA source.
Q. I mean, just looking at this and not normally dealing with these, it looks like there are either more winds or stronger winds blowing south/southeast than there are to the north or -- no, south/southwest, than there are to the north or east. The petals of the rose, if you will, seem to extend out farther. Is that what that means, or am I --
A. I think you are interpreting that incorrectly.
Q. Okay. What does that mean?
A. These wind roses show the direction from where the wind comes.
Q. Okay.
A. So the wind primarily during this period is coming from, essentially, the southwest direction and blowing towards the northeast.
Q. That helps me.
A. The circles indicate probabilities. If you look carefully, you can see the innermost circles, 2 percent, and we have 4 percent, and so forth. So, I think, as I've described in the report, if you just consider the three longest arm -- or take the four longest arms; the first -- it's 8, 10, 12, so roughly 40 percent of the time, maybe more, it's blowing from the general southwest direction.

Q. Okay. You used data from '88 to '92 for this wind rose, but you used data from the '70s for your modeling analysis. Why is that?
A. It's largely a result of convenience and what is available.
Q. If you had this data to put together this figure, wouldn't you have had the data to do your modeling?
A. There are a number of issues that are fairly technical in terms of looking at models and looking at wind roses.

First of all, with respect to models, I think that was also a compound question. The models typically use data which are believed to be broadly representative, and a typical procedure is to select five years of data that we believe are representative.

Representative is determined typically by a meteorologist examining various aspects of the data but, essentially, believe that there is no atypical event.

Given the randomness and the stochasticity, randomness, let's just use that, of meteorological data, generally it's recommended that in a modeling application you use five years of data that are believed to be representative. And the general modeling guidelines from EPA, including national EPA

or federal EPA, as well as Ohio EPA, would normally require that unless site-specific and high-quality meteorological data on-site is available, in which case perhaps less data, less -- one-year duration, for example, could be used.

In my review of the data available on-site, I did not see, first, that the data completeness was good enough; in other words, there were a lot of missing data.

Second, I didn't have sufficient information with respect to the location and the height of the observations. It was not documented on the materials that I had. And then also I understand that the site changed, so for those reasons I opted not to use the on-site meteorological data for this analysis.

However, I did do an analysis comparing wind directions and wind speeds between the available data, which, as I stated, was incomplete to provide a statistically robust wind rose, or other determination, with the Huntington and perhaps the Parkersburg data as well. And they showed general concordance, which I would expect, although the frequencies of calms and others, low-wind speed conditions, did diverge somewhat. Again, nothing

particularly striking in this analysis.
Q. When you say you compared the on-site data with the Huntington data --
A. I believe what I did -- I'm sorry, please finish your question.
Q. No. That was my question.
A. I don't recall exactly which site I looked at, but I did do some comparisons where I plotted the wind direction and the wind speed at the on-site facility versus one of those sites, just to confirm that it did have reasonable concordance and that the data were reasonably reliable, and that analysis produced the expected result.
Q. Now, you say usually data -- you have a meteorologist look at the data to make sure there are no outlying events in the data. Did you have a meteorologist look at this data?
A. No. What was done, and what is typically done, is that the Ohio EPA in this case, the permitting agency, with the purview over modeling applications, will make recommendations for the use of meteorological data with the standard models. And this is what was done with EPA. It's available and cited in the reports, available on their website. And I, essentially, used the recommended data for modeling this facility,

taking the data directly off the website appropriate for the location of this facility.

I, also, as uncertainty analysis or as sensitivity analysis, substituted the data for one of the other sites nearby to see if it would make any difference. And the differences were quite small. Which, again, is no surprise. And so the modeling results aren't particularly sensitive in this case to whether or not you use the Huntington or the Parkersburg or, perhaps, other data.

Q. But when you ran those other data, you did, in fact, choose the model run that gave the highest concentration, didn't you?

A. No, I don't believe so. What I did was run the model -- the model was run -- we are talking about modeling here, but your original question was not on modeling.

Q. Right.

A. I just want to remind you.

The modeling is done for four or five years. And I report the analysis -- the results, the highest concentration predicted for each of those years.

Q. Okay. We will get back into modeling.

On the top of page 8, you mention in the

ambient monitoring that -- with the 2002 data that samples of less than 240 micrograms per cubic meter would not be detected. Do you see that as a problem with the 2002 ambient monitoring?

A. Yes, I answered to you in the affirmative that one of problems with monitoring were the detection limits.

Q. Why is 240 micrograms per cubic meter limit a problem?

A. Well, for a number of the reasons. First, we want to be able to detect levels -- well, primarily we want to be able to detect lower levels.

Second, when you have detection limits which are close to the observations, your uncertainty is typically higher. So I would like to be able to detect materials, pollutants, at lower concentrations and with greater certainties.

Q. How low concentrations would you want to be able to detect?

A. For what?

Q. H2SO4.

A. In this application or in general?

Q. If you were doing ambient monitoring.

A. For this application or in general?

Q. For this application.

A. In this application, I think that getting quite far below levels like the California REL would be

appropriate. And since we are looking also at plume tracking, this is health effects aside, whether we have the plume or not is important to know. Then I would like to get quite low.

And so the approach used here, essentially, workplace-monitoring approach, which is not really that sensitive, and I guess if you want me to quantify how low I would like to go, I suppose getting 10 times below the California REL, on the order of 10 micrograms, something on that order, 20, would be useful.

Q. Are you aware of any monitoring methods that would allow you to reliably detect 10 micrograms per cubic meter, or below, of H2SO4?

A. I haven't researched this recently. There are much -- this method, as performed at that application, was not optimized and, in fact, their sensitivities are at the higher end of things.

I believe I have seen labs that are able to get well below 1 microgram of background reliably, or precisions well below that, which would mean that sensitivities at least three times better can be achieved using the NIOSH methods.

Essentially, any method can be optimized to provide greater sensitivity, and this is something

that we do routinely, not for sulfuric acid, but for other contaminants. And there are a variety of approaches to optimize this method, the NIOSH 7903.

Now, there are other methods that are available as well, and their proof of performance in the field is not as well characterized as the NIOSH method. To become a NIOSH method, you need to undergo a battery of tests and validation. These other methods are not as well developed, but they certainly potentially have good application in -- at the Gavin area.

Q. Are there other NIOSH methods that would allow you to detect ambient H2SO4?

A. Beyond the 7903?

Q. Yes.

A. I don't recall offhand.

Q. When you say "optimize" the process, what do you mean?

A. The sampling phase and the analysis phase have many parameters. Those parameters can be adjusted to improve the sensitivity and this -- I can get into details in that, if you would like.

Q. But that would be in the collection of the sample, or in that -- would that optimization relate to the collection of the sample?

A. It would, as I just indicated, as well as in the

analysis of the sample. And I suppose if we want to break it down into the preparation of the media as well, which is perhaps the problem that was present in some of the 2002 data.

Q. What do you mean by the media?

A. The absorbant in this case, I believe they obtained this from Supellco, comes to the lab or the field personnel in glass tubes.

They break open the ends and then pass air through it. That's the absorbant. I think it's silica gel in this case. And that can certainly be prepared in a far cleaner fashion at somewhat increased cost, relatively small increase in cost.

Q. The third paragraph on this page, you mentioned the shorter sample period in 2002 further reduced H2SO4 plume detections.

This is more of a general question: When you say "plume detections," do you mean plumes that have a measurable amount of H2SO4 in them?

A. Not exactly. What is a measurable amount will depend on the measurement. So -- I'm sorry, I'm trying to find which paragraph here.

Q. It's starts out second. It's the third paragraph on the page. I'm on page 8.

MR. BAILEY: What's the first word in the

paragraph?

BY MS. CRABTREE:

Q. The shorter sampling period, 2002, further reduce H2SO4 plume detections.

A. Okay. And your question is?

Q. When -- it's more generally what you mean by a plume detection. What do you consider a plume detection?

A. When an H2SO4 concentration is detected by the method used.

Q. There could be a plume without an H2SO4 contingent, correct?

A. Sure.

Q. Is it your understanding that sulfur trioxide or H2SO4 changes the behavior of a plume?

A. Not in any significant amount, I would say.

Q. Does it change the chemistry of the plume, other than by virtue of being there?

A. Sure. It's an acidic aerosol and it will have a degree of chemistry that is taking place. Whether it's significant at these time scales seems somewhat doubtful, but there is plume chemistry going on.

Q. Okay. The last paragraph on this page -- I'm sorry, that's not right.

Is it your understanding that just one SCR was operating in 2002?

A. That's my understanding.

Q. You had earlier, I believe in footnote 2, referenced a 14-part-per-million -- it's on page 6, a 14-part-per-million concentration at Gavin unit 2 in 2002 with 6 parts per million at unit 1. In Table 1 you have concentrations ranging up to 2,410 micrograms per cubic meter.

Is it your opinion that the Gavin plant can emit a sufficient amount of sulfuric acid to give a 2,410-microgram-per-cubic-merit ambient concentration with just one SCR running?

A. It's a possibility.

Q. Now I'm turning to 2.2.3, which is on page 9 of your report. This section is entitled AEP is the Dominant Source of H2SO4 Emissions in the Cheshire area. It's not the only source, though, right?

A. No, it's not.

Q. You identify four sources under the bullet points in this. Are all four of those sources a possible source of ambient H2SO4 concentrations in the Cheshire area?

A. I think I would characterize them as known sources of H2SO4 in the Cheshire area.

Q. Did you consider mobile sources, such as cars or trucks, along Route 7 as a possible source?

A. I considered that.

Q. Did you quantify that anywhere?

A. No, I didn't.

Q. Now I'm looking at section 2.2.4, which is entitled Meteorology During Plume Detections Indicates that Gavin is the Major Source of H2SO4. In that, you reference -- in that section, you reference Table 4, and that table is referenced in the second paragraph on page 10.

My first question on Table 4, why are there only 12 days listed here, but there are 38 days listed in Table 1?

A. I provided this analysis for a subset of the data.

Q. How did you determine what that subset would be?

A. I believe I selected the 2002 data and then I selected, I believe, the higher 2001 data.

Q. So, in general, you chose the highest values for this chart, then?

A. No. I said, for 2002 I selected, I believe, all of the data; and for 2001, I believe, I selected the higher data points.

In the second paragraph on page 10, first line, I state, I provide all 2002 data and selected 2001, as I have just indicated to you.

Q. I was just wondering how you selected those two particular dates from 2001.

A. I think I selected the higher concentrations. Then I simply ran out of time to provide this analysis. Also, the results were fairly consistent with what I had looked at, so I stopped at that point.
Q. This is a day-by-day comparison, not necessarily a concentration-by-concentration? I notice in Table 1 there are some days where there are multiple concentrations detected.
A. That's correct.
Q. So when you have prevailing meteorology in Table 4, you are looking at the entire day?
A. I did my best to try to understand when the sample was collected and the meteorology that prevailed during the sampling collection. However, because of inadequate documentation in the sampling, that could not always be done. And then I noted that I believe the consultant was sampling during the daytime up to midnight hours, or something like that, but I believe was unlikely to be sampling in, say, the early morning from midnight onward. I did look at that data.
Q. Did you look at -- well, first, what meteorological data did you look at for this chart?
A. I need to refresh my recollection by reviewing what I have stated in here, Cheshire.
As it states on page 9, the last paragraph

above the footnotes: This analysis was based on surface meteorological data from the on-site station at Gavin as well as the Parkersburg.
Q. The PKB is Parkersburg?
A. That's correct.
Q. All right. Did you look at the Parkersburg or Gavin data to identify periods of calm or light winds where there wasn't an ambient concentration detected?
A. Yes. I did an analysis of the frequency of calms, and I didn't make a determination in that analysis whether or not there were detections.
Q. So those were separate analyses, then?
A. Largely separate analyses, not entirely separate because I do believe I plotted data over this period and looked at a fair amount of data, whether or not there was detections or not.
Q. You conducted -- in this section of your report, you conduct a quantitative comparison of Kyger Creek emissions with Gavin emissions. Did you do any type of quantitative analysis for these other sources of sulfuric acid in the ambient air?
A. I'm a little uncertain what you mean by quantitative comparison.
Q. You went through and did some calculations comparing Gavin and Kyger Creek on page 10. Did you do that

same type of quantitative analysis for any of the other identified sources of sulfuric acid in the area?
A. Did I do the same analysis as on page 10? I'm still uncertain what you are asking exactly.
Q. Let's start with that. Did you do the same analysis as on page 10 with the other facilities?
A. I have to step further back. What do you mean by the same? Same as what?
Q. Same as the analysis as you did on page 10 between Gavin and Kyger Creek?
A. Again, I'm sorry to be so specific. Are you talking about the analysis that is in one, two -- the fifth paragraph?
Q. In contrast to Gavin, the Kyger Creek facility has a taller stack --
A. Okay. No, I don't recall doing -- I don't believe I did any other analysis similar to this, other than the Kyger Creek facility and Gavin facility.
Q. Do you know the megawatt reading for the Kyger Creek facility?
A. I did.
Q. It's a memory test.
A. I don't recall it offhand.
Q. Do you know how many units are there?
A. It has a single stack, I believe, and I don't recall

for certain how many units are there.
Q. Are you familiar with the term "atmospheric stability"?
A. Yes.
Q. What effect does -- first, can you define the term for me?
A. Yes.
Q. Please do.
A. Atmospheric stability refers to the tendency of the atmosphere to provide dispersion or mixing.
Q. Would a more stable atmosphere produce more mixing?
A. In general, no. An unstable atmosphere produces more mixing.
Q. What effect does atmospheric stability have on the plume, the Gavin plumes?
A. Atmosphere stability would affect the degree of mixing of the plume and the dilution that results and, ultimately, is one of the factors that would influence the ground level concentrations.
Q. It would be one of the factors?
A. Sure.
Q. Which would have a greater impact, wind speed or atmospheric stability?
A. There are a couple of parameters, so that question doesn't make a lot of sense.

Q. Okay. What are those parameters?
A. Atmospheric stability is determined in part by wind speed. So when wind speeds are very high, you are unlikely to have stable atmospheres.
Q. So you are going to have a more stable atmosphere when the wind speeds are low?
A. Not necessarily, but you can.
Q. Would there be more of a tendency to have a more stable atmosphere when the winds were low?
A. It's not the way I would like to approach the question. I mean, the stabilities are a result of a number of factors, which include wind speed. And under certain wind-speed conditions, you can have certain stabilities.
Q. What are the other factors in atmospheric stability?
A. The other factors that determine atmosphere stability, is that what you are suggesting?
Q. Yes.
A. If you use, for example, the models that are represented in my report here, atmospheric stability is a classification system based on the sun angle or the time of the day, and the season. The insulation and the wind speed using a system that EPA has codified, I suppose, in their conventional Guassian, G-U-A-S-S-I-A-N, plume models. But there are

alternative typing systems for stability.
Q. When you run the modeling, the type of modeling that you ran, which I understand is the ISC model?
A. There were two models that were run in -- well, actually more, but the ISC is one of the models, yes.
Q. The Screen 3 was the other mentioned in the report?
A. The other Guassian plume dispersion model, that's correct.
Q. You said there were more than two?
A. Yeah, I would consider some of the analyses that I provided later on as modeling types of things, but for your purpose, I think these are the ones you're referring to.
Q. What other analyses would you consider as modeling?
A. Well, I take a model as quite broadly defined. So a model, for example, that we have already talked about is represented in the attenuation factors. This is a type of model.

The -- the examples that I provide later on, looking at dilution factors, is a type of model. This is the sort of things I'm talking about.
Q. I think I understand now.

When you run the Screen 3 -- or the ISC model. Let's start with the ISC model. When you run the ISC model, do you quantify atmospheric stability

during the process of running that model anywhere?
A. No, I don't think I considered quantifying it. We use an input file which has the atmospheric stability typing system determined for us, but we don't quantify it, per se.
Q. Did you look at what the atmospheric stability was during the plume -- the plume touchdown -- wait, I'm starting over.

Did you look at what the atmospheric stability was at the time of the observations in your Table 1? It's the listing of the concentrations.
A. For some of them, I think I did, yes.
Q. What did you find?
A. Well, first of all, Table 4 has the analysis of the ground-level concentrations and prevailing meteorological conditions. And I can look at that, and infer from the light winds the time of the day, the high sun angle, the lack of precipitation, and so forth, at most likely that we were in unstable conditions for most of those events.

I believe I looked at available meteorological data for this, and I can look at the data and indicate whether or not we are likely to be in unstable, neutral, or stable conditions.
Q. It just wasn't an explicit analysis that you provided?

A. No, it wasn't explicit.
Q. Going back a minute. In the paragraph we were talking about where you were comparing Gavin and Kyger Creek, on page 10, about two-thirds of the way down, you say: Screen 3 predicts that Kyger Creek's worst-case impact would be a factor of 2.26 times lower than Gavin, a difference entirely attributable to the additional plume height obtained at Kyger Creek.

If Kyger Creek was a scrubbed plant, would that change your analysis?
A. If you change parameters of the release, then implicit in your question is that scrubbing would change parameters of the release then, yes, the analysis would change.
Q. Okay. Because you had said "a difference entirely attributable to the additional plume height." By plume height, do mean the actual plume or the stack?
A. Oh, I see your confusion, sorry.

The plume height is the combination of the physical stack height. And, as you know, the Kyger Creek stack is taller, and then the plume rise, which is the additional height attained as a result of temperature or buoyancy effects and momentum effects coming from the velocity of the plume. This is referring to the change in plume height, which is the

sum of those two.
Q. Okay. That's what I didn't understand.
This gets back to a question I think I asked earlier but I didn't quite get the answer to. I'm on page 11. It's the first nonitalicized paragraph, the third line down: I selected the five years of PKB data, 1973 to '77, as the longest and most representative meteorological record available for analysis.
Why did you not use the '88 to '92 meteorological data that you used for one of your figures?
A. Yeah, as I recall that figure, to be specific, that was the wind rose which was shown as figure 2, was provided directly from EPA. And I just grabbed that. I don't know for sure that the '88 to '92 meteorological data that that is based on, was available conveniently. That's the primary reason.
Both of these data sets are believed to be, you know, reasonably representative, so probably won't make a large difference one way or the other.
Q. What did you do to compare the climatology in the mid-'70s to the current conditions?
A. I looked at some of the distribution data and, perhaps, some other analyses. I don't recall.

Q. Did you look at other sources to try to get more contemporary meteorological data than the Parkersburg '73 to '77 data?
A. Yes. I had the on-site data, which I compared to -- you can refresh my memory now -- with the Parkersburg data, and that is contemporary with this time period.
Q. With the '73 to '77 time period?
A. No, no, the on-site data is less five years.
Q. Okay. So you compared the '73 to '77 to the more current on-site data which you said before wasn't complete enough to use in this analysis?
A. That's correct. I did some basic distribution statistics.
Q. In your bullet points now, on the second bullet point, you identified July as the month where there would be the most calms. First, can you define calms for me?
A. Sure. Calms are simply defined as a period of time where the wind speed is not high enough to, essentially, to make the manometer revolve. So there is no determination of what the wind speed is. Typically, the wind speed is below a meter or meter and half per second.
Q. So there could be a very, very light wind that's not just being picked up?

A. It would be a very light wind.
Q. Here you say that July would have the most calms during the ozone season?
A. Is that a question?
Q. Yes.
A. Well, I stated in looking at this record of data, in July there were 25 days of calms -- with calms, rather.
Q. In the bullet point before that, the last sentence of that says: The frequency of the calms is the highest in July with 122 hours, 16.4 percent of hours in the month.
A. Yeah. It's slightly higher in July. It's -- as I stated there, it's 12 percent on an annual basis. And July is, as I say in the last paragraph, the last bullet sentence in that paragraph, it's 16 percent, 16.4 percent in July.
Q. So if the calms are most prevalent in July, does it surprise you that only one measurement from 2002 of a detectable amount was from July?
A. No, not necessarily, because there are other things that are involved as well. And plume detections, as I indicated to you first, there were systematic problems with respect to the ability to detect the plume, given the monitoring strategy that was selected.

Second, this is a random process, so the difference between 16 percent in July and, perhaps, 14 or 15, or whatever, percent it is in June when you have relatively small sample size, isn't likely to be detected.
This analysis also doesn't take account of winds shifts and wind directions and operations at the facility, and so forth. So you are looking at only 1 parameter here, and it's not surprising, given the randomness here and the sampling strategy and the problems that are present, that you found more detections in one month than another.
Q. If the SCRs were operated year-round, would you expect to find more ambient concentrations detected in the ozone season than in the nonozone season?
A. You have to clarify a few things for me.
Q. Okay.
A. First of all, the SCRs, if they were operated year-round, the facility could still have levels of emissions of SO3 and H2SO4 that might vary seasonally, and I don't know what that variation would be, premised on your question to me.
Q. I'm assuming the concentrations from the stack were the same year-round.
A. Okay. Assuming the emission rate was the same

year-round, which would be the same as what you just stated, except that in addition to concentrations we would have the same output of a plant, which is likely, as it's a base-loaded facility.

Next, then, the meteorological conditions during the nonozone season vary, clearly. We have wind profiles, wind rose profiles, for example, that will be quite different from what we saw in figure 2. We have wind speeds which are also quite different from what we see in figure 2.

So I, offhand, have not run that analysis to see whether or not, for example, a dispersion model would predict a higher concentration in one season or another. I really haven't focused on that.

However, your question, in fact, was more limited because you were talking about plume detections. And there was, essentially, very limited monitoring outside the ozone season. So, without the monitoring, you wouldn't have any plume detections.

Q. Okay.

A. I take it that my answer would really be contingent on an ability to detect a plume detection.

Q. Assuming that you have same ability year-round?

A. Ability to?

Q. To detect concentrations year-round, can you opine as

to whether there would be more or less detectable plumes in the ozone season without running that more detailed analysis?

A. You said "year-round" and then you said "in the ozone season."

MS. CRABTREE: Can you repeat to me that back?

(The requested portion of the record was read by the reporter)

BY MS. CRABTREE:

Q. I will ask it better. If you had the same ability to detect the plume year-round, would you, in general, expect more plume touchdowns in the ozone season, all other things being equal?

A. Yeah, I think I would, with plume touchdowns defined as occurring in the local vicinity. And the reason for that is that the stability conditions, to come back to that, during the rest of the year when the wind speeds are higher, when the insulation is lower, i.e., it's not summer, so the sun is not overhead, and the likelihood of inversion conditions also is going to be different as well, would probably lead to greater dispersion of the plume and, thus, lower ground-level concentrations.

But I haven't run that analysis to confirm

that, and there is always the chance that you will have an unusual condition in September, something like that, that might produce a high concentration.

Q. Do you believe there will be a plume touchdown every time there is a calm?

A. I would say that is unlikely.

Q. What percentage during the ozone season would you expect there to be a plume touchdown when there is a calm?

A. I don't have the information to provide that answer.

Q. Is it your opinion that any detect -- any plume touchdown is a health hazard?

A. Plume touchdown definition is kind of vague to me. If you can be a little more precise on that, because we have been using plume touchdown to mean a number of different things here.

Q. What do you consider to be a plume touchdown?

A. I think a plume touchdown is an event which causes a high ground-level concentration in the vicinity of the facility extending to some distance. And, technically, I'm a little uncomfortable with touchdown because normal dispersion of the plume would lead to patches or plume borders or boundaries or tails of the plume impinging on the earth surface, or near the earth surface, while much of the plume is still far

overhead. So you can, in fact, produce ground-level concentrations that are measurable even though much of the plume is still well above. And, you know, the common vernacular, you sort of think of a plume as a liquid stream, or something like that, just pouring down to the ground.

Well, in this case a few droplets, if you would like to use that analogy, have dispersed down to the ground. Whether that is a plume touchdown event or not, I'm not hung up on the semantics.

From the health-science perspective, I care about what is the ground-level concentration, whether or not you call it a plume touchdown.

Q. Okay. Let's call it a high ground-level concentration. How high a ground-level concentration would you consider a health hazard?

A. In the report, I go through a number of different concentration ranges that might be considered as benchmark values, if you will, or guideline values, and something on the order of 100 micrograms per cubic meter is, say, a level of concern, something on that range. I'm sure we will talk a lot more about that.

Q. We will get there.

A. For the purpose of just moving our discussion here, I'm just going to provide that number. I don't want

to be pinned to that particular number at this point, but just to move things along.

Q. In your opinion, is any ground-level concentration of 100 micrograms per cubic meter or above a cause for concern?

A. It could be, yes.

Q. Under what circumstances could it be?

A. Well, if there is an exposure that's occurring, it would be of concern.

Q. But if there was no one there, it wouldn't be of concern?

A. If there is no one there, it would not be a health concern.

Q. A tree falling in a forest?

A. To humans.

MR. BAILEY: You have to define no one.

MS. CRABTREE: No humans.

BY MS. CRABTREE:

Q. Do you have any understanding of the future operating plans at the Gavin facility?

A. Yes.

Q. What's your understanding?

A. My understanding is that the facility will continue to operate; that they are changing out the catalyst in the spring, I think, of this year in one of the units;

and other than that, it's business as usual, I believe.

Q. What about year-round operation of the SCRs, do you have any knowledge/understanding of whether that will happen?

A. I haven't seen anything specific that would -- on that matter. My understanding is that the SCRs will operate during the ozone season, and it's -- I have no specific information with respect to Ohio EPA's policy on that.

Q. Okay. Why would that implicate Ohio's EPA policy?

A. My understanding is that it states monitoring ozone during the ozone season and are, in fact, somewhat reluctant to extend monitoring into the nonozone season.

It's the state's monitoring that determines compliance or noncompliance with the national ambient air quality standards. States are not particularly eager to increase their costs of monitoring and, in particular, have the possibility discovering they are not in compliance during the nonozone season.

MS. CRABTREE: I'm at a good stopping point before we jump in dispersion modeling, if we want to take a quick break.

MR. BAILEY: Sure.

(A recess was taken)

MS. CRABTREE: Back on the record.

BY MR. CRABTREE:

Q. I want to move now toward the dispersion modeling section of your report, section 2.3. How often do you engage in modeling of this sort. By this sort, I mean dispersion modeling.

A. Frequently.

Q. How frequently?

A. I have probably modeled hundreds of facilities in applications over the years.

Q. Do you run the models yourself?

A. Not necessarily.

Q. Do you usually run the modeling yourself?

A. Sometimes I run the model myself; sometimes I have students run it.

Q. Did you run this model yourself?

A. I believe I ran this one myself.

Q. When you do dispersion modeling, do you usually run the ISC model?

A. Often; not exclusively.

Q. What other models, dispersion models, do you use?

A. ISC is being phased out in a bit, so we are using Air Mod now.

Q. That was November of last year that Air Mod was

approved by EPA or added --

A. No, it's been out there for a little while, but in terms of being an approved or recommended guideline model, I don't know the exact date when the EPA has made that recommendation.

Q. What time period did you conduct your ISC modeling?

A. In the fall last year. I don't recall the exact date.

Q. Other than the ISC model and Air Mod, do you use any other dispersion models?

A. Yes, there are a variety of different models. I mean, Caline is another model, for example, C-A-L-I-N-E.

Q. Why didn't you run Air Mod on the Gavin data for this case?

A. First of all, I was just interested in replicating the analysis that was performed for Gavin and approved by Ohio EPA.

Second, I had the input files, or very close facsimile to the input files, making the application quite easy to run.

And, third, it's a fair amount of work to port it over to another model, and resources weren't available to me to do that.

Q. Do you have any sense of whether your results would have been different if you had used Air Mod?

A. Yeah, they probably would differ some. I haven't run

the Air Mod in this application, so I didn't know offhand how much they would differ.

Q. Do you know if they would tend to predict higher or lower concentrations?

A. I don't know that offhand. I haven't run the model, as I stated.

Q. You said you had the input files for the ISC models. Were did you get those from?

A. That was on some of the electronic files that were obtained in discovery.

Q. So they came from the defendant?

A. Well, they came via Jim Hecker, wherever he got them from.

Q. I think I know where they came from then.

The first nonitalicized paragraph, 2.3, page 12, you say that you used a stack gas concentration for SO3/H2SO4 of 17.5 ppm. That's parts per million, right?

A. That's correct.

Q. Why did you use 17.5?

A. I think what I tried to do is to essentially replicate the modeling that was performed by Ohio EPA, and as stated in the same paragraph that you just quoted, I believe that provided the emission rate of approximately 122 grams per second at each stack.

When you convert that to a ppm concentration, you get approximate 17.5.

Q. You said that you tried to replicate the model, did you ever see the output files from that modeling?

(Discussion off the record)

BY MS. CRABTREE:

Q. Go ahead.

A. The question was did I ever see the output files. I saw output files of dispersion modeling runs provided by Jim Hecker. I don't recall offhand whether those were run by Ohio EPA or by the Ohio Power, but they were outputs that I did see.

Q. Did you compare your outputs to the output files that you had?

A. These files were very large. I compared part of them, anyway, to what I had.

Q. Did they generally match those other output files?

A. I think they did.

Q. Why didn't you just use the output files that you had?

A. Well, first, I'm interested in replicating the data to provide some degree of assurance that things are okay.

Second, I'm also interested in working with the data a little bit, and the output files that were provided are in a -- I'm not sure if they were in a PDF format, or whatever, but it wasn't very convenient

to work with and compile some of the statistics.

Third, I'm interested in providing some degree of sensitivity analysis to see, as we discussed earlier, what would be the impact of changing from Parkersburg to another meteorological database to see, in fact, if results are abnormally sensitive to an input. I think those are the major reasons.

Q. Okay. You note on the last paragraph on this page: The highest one-hour H2SO4 concentrations over the four-year period range from 109 to 130 micrograms per cubic meter.

You mentioned earlier that usually you use five years of meteorological data. Why did you only use four years here?

A. Yeah. I think what happened was for a period of time I could not locate the fifth year of data. That's why that was not run. I think subsequently I may have located that data, and it doesn't change the results in any substantial way, but I think that's what happened in that case.

Q. You say: The highest one-hour concentrations range from 109 to 130 micrograms per cubic meter.

Does that mean 130 micrograms per cubic meter would be the maximum expected ground-level concentration over a one-hour period?

A. That is the maximum predicted one-hour concentration over that simulation period of four years.

Q. Going back to your Table 1, the second to last column, it looks like all of the concentrations in micrograms per cubic meter for the 2002 data are well over the 130 micrograms per cubic meter, but they all tend to have close to a one-hour duration. Did that cause you concern as far as the validity of your modeling outputs?

A. No.

Q. Did it cause you concern as to the validity of the 2002 data?

A. The monitoring data?

Q. Yes.

A. Not directly, no.

Q. Why?

A. As I explained at some length in the report on pages 13, I think, and 14, and on page -- well, extensively limited -- largely limited to those two pages, there are a number of limitations in the dispersion modeling that can cause the predicted concentrations to underpredict the concentrations that occur in the vicinity of the Gavin facility.

Q. Now, can you turn to Table 5 for me, please, in the back of your report. This table is labeled predicted

highest and second highest one-hour and 24-hour H2SO4 concentrations from 1974 to '7 disperse modeling.

What I didn't understand was under the concentration column, it says micrograms per cubic meter. At the bottom, you have an average of 10.4 and 6.6. What do those numbers mean?

A. Okay. If you look under the entry one-hour concentrations --

Q. Uh-huh.

A. -- then we have the first and second high for the four years, so there should be eight rows of data there.

Q. Yes.

A. Then there is an average 1 and 2, so that represents the average high and the average second high for the one-hour data.

The next eight lines are the 24-hour data, and following that we have the average 1 and 2, and that's the average highest and second highest 24-hour concentration.

Q. So your average 1 is an average of all of the ones labeled 1 in the lines above?

A. That's correct.

Q. Okay. I just wanted be clear --

A. For only that subset. Maybe a line between the 1-hour and the 24-hour parts would help you.

Q. I just drew one.

A. Okay.

Q. So the average concentration -- one-hour concentration at the parameters that you modeled would be 121.1 micrograms per cubic meter?

A. That's the average of the four highest, the annual -- the annual maximum.

Q. And the average of the 24-hour concentrations, the highest ones, was 10.4 micrograms per cubic meter; is that correct?

A. That's correct.

Q. Why is the 24-hour concentration so much lower than the 1-hour concentration?

A. The answer is because the wind is unlikely to be in the same direction for all 24 hours of the day, and the other factors that affect dispersion are completely unlikely to be the same and, thus, you have basically the plume going in all different directions over a 24-hour period, but the average 24-hour concentrations is taken only at one location. So you are essentially just not following the plume. You are at just one location.

Q. All right. Going back to your text, a question about footnote 8, on page 12, just to go back for a moment. Footnote 8 -- hold on one second.

This actually goes back up to the paragraph above, 2.3, you say: 2004 TRI data shows a large increase in emissions over 2002.

Do you know if one or both SCRs were operating in 2003?

A. I think footnote 8 states that both units had operating SCRs in 2004 and only one operating SCR in years 2002 and 2003. I think that's correct.

Q. You assumed just one SCR in 2003?

A. That's correct.

Q. Okay.

A. But I don't have any data from 2003 anyway, other than the TRI data. So it's kind of irrelevant to the answer.

Q. If you have calm surface winds at ground level, does that mean you will have calm winds at stack height?

A. We talk about calms as winds that are not measurable at the height of the instrumentation. And certainly you can have different conditions prevailing aloft. But we don't talk about calm surface winds because calms are a no-win condition and winds are a windy condition, so it's kind of an oxymoron.

Q. Would winds tend to be heavier at plume height versus ground level? Or "higher" is probably the better word.

A. The velocity of the wind is normally higher as you go up, or at least not lower, due to diminished coupling, but it's certainly possible to have calms aloft as well.

Q. On page 14, at the very top there is a bullet point there, and actually preceding text to that, there is a bullet point before that on page 13, and you are talking -- this is in a section called dispersion modeling has major limitations. There you say: The periods of less than one area -- excuse me.

Periods of less than one hour are not modeled; thus, precluding comparison with short-term occupational standards and guidelines.

Do you see where I'm at?

A. Page 14?

Q. Yes.

A. Yes.

Q. But later in your report, you had recommended -- on page 21 of your report, the second bullet point under 2.6.1, you specifically recommend a one-hour emission limit. I guess my -- a short averaging period, excuse me, specifically to one hour.

Why do you say in this section that an averaging period of one hour is recommended, but here you say that there needs to be a shorter averaging

time with the modeling?
A. I think you can get more stringent and, in fact, require a shorter averaging period for monitoring that would be fine, that would preferable.
In general, there would be relationship between a 15-minute emission number and a 1-hour emission number if you have an excursion during a 15-minute period that is going to be reflected in a higher 1-hour average as well.
So, yeah, I mean, if you had a, you know, the availability to take even shorter time period for monitoring that would be preferred.
That's typically done with things like opacity limits, for example, which might be specified as short as seven minutes. So if you could do that, that would be preferable. Probably one hour will provide a degree of protectiveness, which is suitable.
Q. But the shorter the averaging time the better, in general?
A. The better would depend to the attribute of concern. It's challenging, more challenging, to take high-accuracy measurements with a shorter averaging time. It would be more expensive, and so forth. So, overall, I'm trying to provide some balance between what I think is a reasonable balance between being

protective and being feasible and technically responsible, and so forth.
Q. Back to page 14, on the third bullet point at the top, before we get to 2.3.3, you have: The dispersion modeling assumes a constant emission rate in near maximum stack gas exit velocity over the modeled period. As discussed below, this is clearly not the case, as much higher missions and lower exit velocities regularly occur, both of which will increase ground-level concentrations.
Have you simulated this in your modeling, where a higher emission -- higher emissions are associated with lower exit velocities?
A. No, I don't need to simulate that. The output of the model is proportional to the emission rate. So if, in fact, I know that emissions go up by, say, 10 percent and the ground-level concentrations will go up by 10 percent, I don't need to simulate that.
Q. Have you ever seen a situation where lower loads on a unit produced higher ground-level concentrations?
A. For Gavin?
Q. Ever.
A. I can't recall for ever. Certainly we can run the modeling and simulate that the effect would, in fact, produce a higher concentration if the emissions go up

more than the reduction due to the decrease in load.
Q. If the emissions go up more than the reduction than the decrease in load --
A. In other words, at Gavin, for example, there are cases which I think are specified that look further in this section where at nighttime, for example, there is a decrease in load, and I think I specified where this is.
Q. I think you are talking about the first bullet point in the footnote in 2.3 --
A. That's correct. I didn't look at the footnote. So the volumetric flow decreased, as it says in the footnote, from 25 to 40 percent; but, in fact, the ppms at the stack increased a larger percentage than the decrease in load.
The decrease in load will reduce the volumetric flow, which will reduce the plume rise, and so forth. And I didn't simulate this. The model was run with a constant emission rate. But in this case, this is an example of where that might happen.
Q. You based these numbers and this analysis on this Excel spreadsheet that is identified in the first nonitalicized paragraph of 2.3.3?
A. I believe I did.
Q. Do you know what that spreadsheet is?

A. It's a spreadsheet giving parameters of emission rates during that particular year.
Q. Did you research any of the parameters in the spreadsheet, or was it just an output in parts per million?
A. Well, I researched -- I don't know exactly what you mean. But I looked at the spreadsheet, and it indicated over a period of time, which I don't recall exactly, perhaps it was most of the 2004 ozone season, estimates of what the SO3 concentration would be in the stack on an hourly basis. I think it was actually sheets in that spreadsheet that included the two units at Gavin.
So I'm aware that it's not monitored on an hourly basis. In fact, SO3 is monitored infrequently at the facility. So that is a modeled or estimated concentration of SO3, I'm aware of that.
Q. So it wasn't based on any kind of continuous emission monitor or --
A. As you are well aware, there is no continuous emission monitor of SO3 at this facility.
Q. Is there any available continuous emission monitor for SO3?
A. There are technology that promise, and perhaps can deliver on, SO3 measurements in a near-continuous

manner. As I'm aware also, I believe, in 2001 an attempt was made to utilize one of these at Gavin and, for a number of technical reasons, it didn't work out very well. I don't believe that is a proof of instrumentation or technology, for quite a few reasons. We can get into that as well.

Q. Actually, I would like to.

You said they tried a continuous emission monitoring in 2001, but it didn't work out. Do you know what that was?

A. What the technology was?

Q. What the technology was.

A. I believe they used FTIR, which is Fourier Transform Infrared Spectroscopy, F-O-U-R-I-E-R.

Q. And, to your understanding, why was that effort ceased?

A. I'm sorry?

Q. To your understanding, why was that effort ceased?

A. Ceased?

Q. Ceased.

A. I think it was a trial, short-term trial, a couple of weeks perhaps, and there were, sort of, no end of technical snags, that meant results were unacceptable to, I guess the client, being either AEP Ohio Power or EPCRA. I'm not quite sure who was the sponsor of that

particular trial.

It looks, in examining a little bit of the data, a little bit of the results, that it wasn't what I would say the best effort that could be put forward.

Q. Why do you say that?

A. Well, I don't recall all the details, but they had, for example, issues with the sampling line melting, with condensation problems, with other kinds of, you know, fairly basic but troublesome issues that resulted in malfunctions and failures. And had they thought a little bit more about it, I think you would be able to avoid those types of problems.

Q. Are you aware of any coal-fired electric utility in the United States that uses a continuous emission monitor for SO3 or H2SO4?

A. I haven't researched that. I did see EPCRA has had a program to try to evaluate those various technologies. I think they focused on FTIR, but I can't say that I know what is happening across the U.S., as is pretty well understood. I mean, it's not been a concern until recent years.

It takes some time to develop a technology that is robust or hardened enough to be able to provide the kind of performance that you like to see.

On the other hand, the promise of

technologies like FTIR can certainly be applied in a setting, if you are willing to spend the time and money and resources to optimize that and make them work. So just because there may not be a facility with continuous monitoring, it doesn't mean that the technology doesn't exist.

Q. Also on page 14, the third bullet point under 2.3.3, you mentioned excursions well over the 20 parts per million target are common.

Where do you get that 20 parts per million is a target?

A. In fact, in further review of materials that have become available since this report was put together, in particular, I guess, the deposition of Rob Osborn, he talks about targets per stack, per year, that vary from this target.

This, I believe, I got by using the 2001 data, which talked about 19 to 22 ppm as being a target, and so on.

I believe on some of the PowerPoints in the other materials we looked at, there was explicitly stated a 20 ppm, but I didn't document, unfortunately, where that 20 ppm came from. But we are in the realm of the range of things that are consistent with this case.

Q. You said you reviewed Rob Osborn's deposition transcript?

A. I skimmed it fairly quickly in the last couple of days.

Q. Did you look at anyone else's deposition transcript?

A. Yeah, as I stated on the outset here, I reviewed a few of the other depositions as well.

Q. Do you remember the names?

A. Let's see, the plant manager. His name -- I'm going to need some help here.

Q. I can give you names and you can tell me whether or not you have seen the deposition.

A. Sure.

Q. I won't try to make it harder. Jeff Lytle?

A. I skimmed his deposition.

Q. Jeff Navotny (phonetic)?

A. No, I don't believe I have seen that.

MR. BAILEY: We just got that.

BY MS. CRABTREE:

Q. I do think we just got that. We are still missing one, which I know you couldn't have seen.

So Rob Osborn, Jeff Lytle. Did you look at any of the CAP member depositions?

A. No, I don't think I have seen those.

Q. That would be Stinson, Mulford or Thompson.

A. In the past couple of weeks, no, I haven't seen those.
Q. Okay. On page 15, you say that your modeling is not intended as a substitute for advanced modeling techniques that can represent plume touchdowns or more complete monitoring.

What other modeling techniques would you use to more accurately model those things?

A. In this report I didn't get into detail and didn't do a review of the kinds of modeling techniques that are or could be appropriate.

What is fairly clear is that the conditions that cause plume to descend to or near the ground in a way which deviates from a Guassian plume behavior is represented in a model, like ISC, requires some advance type of modeling technique.

Some of the computational fluid dynamic, CFD, models, for example, might be able to do that.

These are fairly advanced types of applications and well beyond the scope of what I would be able to prepare for a report like this.

Q. How much effort would it take to run a CFD model?
A. It's hard to answer that. First of all, it requires probably better meteorological data on-site than what we have. And so, the first step would be to develop, you know, better database.

After the data exists, then it's a fairly straightforward process, but a time consuming and involved one, to develop the kinds of flow fields and dispersion parameters that you need. But probably most critically what you would like to have is some validation that the model works as you intend it to work. So that requires additional monitoring as well. So all of those things add up to a fairly extensive effort.

Q. Would you need to actually go out and do more monitoring for that type of a model?
A. Strictly speaking, you can run a model without additional monitoring. But, as I indicated, I would like to be able to validate that the model -- as the models get more complex, I like to have more assurance that they work properly. So monitoring is one of the better ways to get those assurances.
Q. I'm sorry, flipping back to 14 for a second. In the footnote 9, you have: As nighttime dispersion is often reduced from daytime, this may cause a triple whammy.

What do you mean by nighttime dispersion is reduced from daytime?

A. Unstable conditions generally exist during the daytime when we have solar heating heating the ground surface,

causing a larger temperature gradient, conditions we refer to as superadiabatic, S-U-P-E-R-A-D-I-A-B-A-T-I-C.

These are likely responsible for some of the touchdown conditions, or plume touchdown conditions, in the fumigation case that we see at Gavin facility.

At nighttime, those conditions are generally less likely. We have meteorological conditions prevailing that are characterized as stable conditions, typically, where there is relatively little dispersion.

In most cases, that will keep the plume confined to a fairly narrowed vertical extent, meaning that since the stacks are quite tall, if the plume doesn't come to the ground level, the concentrations are, essentially, in a fan or in a plume that is well overhead, not reaching to ground level.

However, the topography has some rises in the area and there can be conditions that still cause a degree of subsidence in the plume. If that were to happen, then the conditions could be higher at nighttime.

It's not unusual to find for elevated point sources like Gavin, that the highest ground-level

concentrations occur at some distance from the facility at locations of higher elevations.

In those types of facilities, or those types of cases, often higher concentrations occur at night.

Now, at Gavin, it's more complicated. We have conditions occurring during the daytime that bring the plume, due to these fumigation, or other conditions during unstable classes, to the ground that cause high concentrations that we have noted during the daytime. But, of course, there hasn't been much nighttime monitoring anyway, and most of the concern has been very close to the facility, within a few miles or kilometers, 10 kilometers, something like that.

Q. You mentioned fumigation case, can you explain to me what that is?
A. Yeah. Fumigation case might be thought of as atmospheric conditions that essentially cause rampant downward dispersion, bringing the plume to the ground with limited dispersion above the stack height. So, essentially, the plume is concentrated more at ground level.
Q. Do you know what the current H2SO4 level from Gavin stack is?

A. In 2006?
Q. 2005.
A. You mean last year's numbers?
Q. Yes.
A. Target levels?
Q. Actual levels.
A. No, I don't believe anyone really knows. The monitoring is not continuous, and I haven't seen any specific monitoring for 2005. Based on the targets that I have seen, the concentrations, I think, are on the order of 10 to 14 ppm. And I think that comes out of Osborn's deposition, but I haven't seen any backup of that, so that's his opinion.
Q. Now, on page 15, in the second nonitalicized paragraph, the first sentence of that: SO3 stack concentration of 17.5 parts per million is equal to an ambient H2SO4 concentration of about 70,000 micrograms per cubic meter.
A. I'm sorry, which -- oh, paragraph 3.
Q. Yes. You refer to this as an ambient H2SO4 concentration. Wouldn't this 70,000 micrograms per cubic meter actually be the concentration at the stack outlet?
A. Sure.
Q. That would disperse before it would touch down, if at

all, in the surrounding area, correct?
A. Ideally, we like a lot of dispersion.
Just to clarify one thing on my previous answer with respect to what the actual emissions are, I think it's important to recognize that these emissions are likely to vary from hour to hour and season to season, and so forth.
When I indicated that I -- in my review of Osborn's deposition, that he said the targets were 10 to 14, this is sort of a long-term average that represents, again, a predicted output. So fluctuations are going to occur from that number on a short-term basis that will cause levels potentially to be considerably higher or, possibly, considerably lower.
Q. That 10 to 14 he designated was during the operation of SCRs; is that correct?
A. That's my understanding.
Q. Okay.
A. But that 10 to 14 is not the range I'm talking about; rather, is the estimate of the expected output from one stack and from the second stack.
In other words, short-term fluctuations can cause it to, say, go to 20, to 30, or go to 5, but on the long-term average, his opinion is that the average

is 10 at one facility and, I think, 14 at the other.
Q. I understand. Paragraph 5, you mention a mixing height of 300 meters. Where does that come from? Sorry, I'm in the paragraph that starts: The first approach utilizes --
A. Right, yeah. I selected a somewhat higher than the Gavin stacks.
Q. Why?
A. I'm illustrating here the magnitude of the emissions and the need for a high dilution factor to produce ground-level concentrations that are very low. These are a number of examples to illustrate the kinds of scenarios that can -- well, there are number of scenarios that are designed to illustrate the dilution factors that we have. So I'm picking numbers that I think will illustrate those points.
Q. Would you expect that to be the actual mixing height?
A. No.
Q. What would you expect to be the actual mixing height on a high-ozone day?
A. Mixing height also vary over the day and night, and over the season. And the answer is, it depends on many factors. In general, in this part of the region in the summer, morning mixing heights are probably on the order of 1,000 meters, something like that.

Later in the day -- actually, that's kind of a afternoon-time measurement.
At nighttime, they will drop. In the afternoon, they will rise. And I don't recall the exact number. It's going to depend on the site that you look at, but it -- you know, that's an average.
For a particular period of time, it can certainly be much lower. You can have mixing heights that are 300, 200 meters, whatever. So it's going to vary. But the expectation is, thankfully, we have a much higher mixing height, otherwise we would have very severe pollution problems very frequently.
Q. The bowl that you described that the plants are kind of sitting in, for purposes of illustration, it seems like you are using what you call a simple closed-box model. In actuality, wouldn't winds be flowing through that bowl?
A. Yes, exactly. That's the second -- the next paragraph --
Q. Okay.
A. -- where I assume wind is blowing through the box.
Q. Okay. Where you say -- where it says the third approach, the last paragraph on this page, you said: The necessary dilution factor of 583 is obtained by increasing the cylinder's diameter to 309 meters.

Is it your opinion that the diameter of the stacks at Gavin should be increased?
A. No.
Q. Okay. Obviously not to 309 meters, but at all?
A. They could probably accommodate that on the site, but it's not at all the intent to do anything like that at all.
Q. Again, this was just illustrative?
A. No. I think you are misinterpreting what I'm trying to do here. The idea here is to visualize the plume as leaving the stack, which is 12.8 or 42 feet in diameter and, essentially, turning horizontal. So it comes out vertically and then turns horizontally.
And just as a way to imagine what the plume would look like and how large the plume would have to be to produce, let's say, acceptable ground-level concentrations. It's just a way to illustrate the dilution ratio that is needed.
Q. Okay.
A. Increasing the diameter to that magnitude would cause, in fact, things to get much worse.
Q. Why do you say that?
A. Well, assuming you had the amount of cement, and everything else that it would take to build a structure like that or, I guess, that would be a

stadium, with a very narrow aperture, you would be looking down, releasing a plume, then the velocity that comes out that plume would be miniscule.
As soon as it would come out, it would be picked up in what we call downdrafts, or plume downwash, we would likely bring it down to the ground; and that's exactly the wrong thing to do.
Q. Is it your opinion that the stacks at Gavin should be higher?
A. I didn't offer an opinion with respect to that.
Q. Okay.
A. In this -- in this description here, these are examples that are using what is there at present. Increasing a stack height, of course, would tend to lower ground-level concentrations. But then you have GEP concerns to deal with at EPA.
MS. CRABTREE: It's 5:00 now. I have about 20 minutes until we would get to health effects. Do you want to go that far or -- I'm at a stopping point now, if you want.
MR. BAILEY: Are you ready to stop?
THE WITNESS: It's overtime for me.
MR. BAILEY: You got it.
THE WITNESS: I don't remember.
MR. BAILEY: Are you progressing as you

expected?
MS. CRABTREE: Yes.
MR. BAILEY: As long as it doesn't affect your time to end tomorrow.
MR. BAILEY: Off the record for a second.
(Deposition adjourned at 5:10 p.m.
Signature of the witness was requested)

CERTIFICATE OF NOTARY

STATE OF MICHIGAN)
) SS
COUNTY OF OAKLAND)

I, Karen Klerekoper, a Notary Public in and for the above county and state, do hereby certify that the above deposition was taken before me at the time and place hereinbefore set forth; that the witness was by me first duly sworn to testify to the truth, and nothing but the truth; that the foregoing questions asked and answers made by the witness were duly recorded by me stenographically and reduced to computer transcription; that this is a true, full and correct transcript of my stenographic notes so taken; and that I am not related to, nor of counsel to either party nor interested in the event of this cause.

Karen Klerekoper, CSR-4250, RPR
Notary Public,
Oakland County, Michigan

My Commission expires: 10/7/06

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Date: 6/19/06

TO : Molly S. Crabtree
Porter, Wright, Morris, and Arthur

Re: CASE: Cap v. Ohio Power Company

DEPONENTS: Stuart Batterman, Ph.D

DATE: 1/5/06, 1/6/06

Enclosed please find the original transcripts of the above deposition(s) and attendant exhibits, if any.

☐ The errata sheet and signature page are enclosed.

☒ There has been no response from the witness. We are forwarding the original to you, assuming that signature has been waived.

Sincerely,

Patricia Murray & Associates